Review of revision of "Evidence of the complexity of aerosol transport in the lower troposphere on the Namibian coast during AEROCLO-sA" by Patrick Chazette et al.

Recommendation: This manuscript still requires major revisions, particularly in the analysis of the HYSPLIT trajectories. Importantly, the evidence presented that smoke from South America could play a major role in the region remains unconvincing.

Major issues:

The major issue with the manuscript remains the presentation and interpretation of the HYSPLIT trajectories and circulation patterns more generally.

Showing vertical ranges for all three periods together is a major improvement but does not obviously support the interpretations the authors prefer. In particular, looking at the 5000-6000 m range, it is not obvious that South American contributions should be expected in P3 but not in P2 or even P1.

One potential problem is that many dates and vertical levels are convolved in the presentation in addition to the ensemble "uncertainty" around each trajectory. Thus, it is difficult to know if certain regions of the cloud of probability are very certain to have been the source at certain time periods but not others or whether the meteorology was fairly steady but there is great uncertainty about the path taken.

In addition, it is impossible to tell the vertical level of the trajectories through time in this presentation, which could be particularly important for the South America assertions. Running some ensemble back trajectories on the online HYSPLIT portal (see below), I see that a few ensemble members dip down to ~4 km over South America before reaching Henties Bay on September 7th but the majority of ensemble members remain above 7 km. It would be surprising if the South American smoke were lofted that high.

The authors do not adequately explain how they are reaching certain conclusions from the evidence presented. For example, an assertion is made that during P1 trajectories from the 3000-5000 m range show more air masses coming from the southern Atlantic Ocean, and indeed South America, as compared to the other vertical ranges, but this is does not appear to be supported by Figure 13a-c. Similarly, the direct link between the circulation and aerosol transport in Figure 14 is either not obvious or directly contradicted by the figure (in that the coherent area of high AOTs seeming to come from South America stays far south of Henties Bay).

One potential remedy would be to restructure the transport section as separate case studies of emblematic days during the three periods. This should simplify things down enough to show more clearly whether the source is particularly uncertain or if there's just variation in source within the periods and, importantly, the vertically-resolved and

time-resolved trajectory data that would aid in interpretation. This could be provided in addition to or in lieu of Figure 13 now, with a short, added discussion of how the days not highlighted compare. I'm open to other possibilities as well, but feel that the analysis as-is does not merit publication.

A final major issue has to do with figure quality. Many of the scales are all but illegible based on their small size and low resolution (see, e.g., the color bar in Figure 13 and the wind barbs in Figure 14). One or preferably both issues should be addressed prior to publication.

Specific comments:

1. Page 2, Line 54: Rather than claiming that stratocumulus are the most effective at reflecting sunlight (deep convective clouds have higher albedo but smaller net radiative effect due to compensating longwave heating), it would be more accurate to say something along the lines of: "marine stratocumulus are particularly sensitive to aerosol perturbations due to relatively low background aerosol concentrations (Oreopoulos and Platnick, 2008)"
2. Page 8, Lines 180-181: It makes sense that the AOD retrievals over the ocean surface will be more certain than those over land, but this still doesn't address any issues relating to the lack of co-location. Unless the sea breeze is acting uniformly from the surface to ~5 km, it is plausible that the AOD over the ocean may differ somewhat from that at Henties Bay. I would think the effect is small, but it may be worth mentioning as a source of uncertainty regardless.
3. Page 8, lines 213-215: Although no precipitation was observed at Henties Bay (which is unsurprising), it is possible that wet scavenging could have occurred closer to the source of the emissions, depleting the aerosol plume before that air was transported to the Henties Bay area.
4. Page 10, lines 313-314: I'm not sure that "pyro-convection" accurately describes the strength of the primarily anthropogenic plumes in the region. Also, is there significant burning in the Etosha Pan itself?
5. Page 22, line 444: Which altitudes? You're referring to 3000-5000 m here, right? As written, it sounds like you're referring to the full 1500-6000 m column.
6. Page 22, line 450: It is very hard to tell from the figures that the trajectories are turning counterclockwise. Perhaps some kind of composite trajectory would be useful? I'm thinking of the analysis in Adebiyi & Zuidema (2016), Figure 17, as inspiration here. Then you could address the altitude of the trajectories as well, which is not possible to do in the current format and could be important.
7. Page 22, lines 454-455: I don't see how you're concluding that the 3000-5000 m level is "mainly" influenced by air from over the southeast Atlantic as compared to the other two vertical ranges.
8. Page 22, lines 455-456: Some of the ensemble members show the starting location as southern Brazil, but others appear to disagree. It's plausible that the origin was around Brazil, but the trajectory analysis doesn't show that Brazil *was* the origin.

Especially when you've gotten out to 6 days, the HYSPLIT trajectories need to be taken with a generous helping of salt.

9. Page 22, line 456: The aerosol plume is located between 1500-3000 m during P1 according to all the other plots… why are you saying there is no aerosol in that range now? There certainly does not appear to be a plume between 3-5 km during P1, which would be the implication of this section…
10. Page 23, line 462: Most of the fires are anthropogenic and set for agricultural purposes, not "wildfire." "Biomass burning regions" may be better phrasing.
11. Page 23, lines 464-646: For a given start time, the HYSPLIT ensemble provides an estimate of meteorological uncertainty, and while it may be helpful to think of some mixing of airmasses on the way to Henties Bay, a more straightforward interpretation of the ensemble may be that more oceanic and more land-based transport pathways are both plausible. It's problematic to assume that all the trajectory ensemble paths were actually followed, however. This analysis is more appropriate for a plume dispersion analysis, which can be done with HYSPLIT or with another program like FLEXPART. However, if in analyzing individual days within the period you see some with different sources, and that is the source of the wide range in the cloud of probability, that is worth reporting. If true, however, this casts doubt upon the ability to tell a coherent story about three discrete periods, as the paper currently attempts.
12. Page 23, line 473-474: It would be really helpful to somehow indicate times on a figure. Again, see the note about a composite trajectory above. You could even group ensemble members with similar paths together to make things clearer on the map.
13. Page 23, line 476: Certainly most trajectories aren't coming from the south below 5000 m. Also, it's not clear that the amount of trajectories coming from South America differs substantially between P2 and P3, with P2 perhaps seeing even more from South America.
14. Page 23, line 478: The analysis presented does not establish that the highest AOTs on these days are associated with biomass burning from South America.
15. Page 23, line 484: Why is the cloud cover "important"?
16. Page 23, lines 496-497: What is the evidence that the temporal variability of South American transport "appears" linked to the SAM? Are you simply saying this is a plausible explanation because the SAM is generally important, or is the analysis above evidence in support?
17. Page 24, lines 521-523: The relationship between transport patterns and the SAM is plausible, and appears to be very easily testable given datasets like ERA already discussed by the authors. The authors should test this claim (that different SAM phases correspond to their periods P1-P3) if they want to report it. If the authors are unable or unwilling to provide further support for their linkages between aerosol transport and the SAM, it would be best not to include this section at all or merely mention it as an avenue for future research.
18. Page 25, Figure 13: It would be immensely helpful to the reader to have better labeling here, perhaps for both altitude and period (labeling columns/rows would be

fine). It could also be helpful to include an indication of whether smoke was present at the given altitude for each period.

19. Page 26, Figure 14: I don't see how the AOT in this figure supports your conclusions. If anything, it appears to show the South American-linked AOT stays south of 30 S.
20. Page 26, lines 548-549: It doesn't really make sense to say that this is the first time biomass burning aerosols were characterized by lidar at Henties "during the different periods of transport" — it's the first time, period, that (ground) lidar-based characterization was possible. That there were three transport periods is a separate idea (and the division into three periods is an interpretation of the data, not a direct observation).
21. Page 27, line 570: No evidence is presented that the transport regimes the authors associate with periods P1-P3 are the "main transport regimes across the Atlantic Ocean."
22. Page 27, line 577: I still think it is overstating the case to consider a 10-15% contribution to column loading seen on two days out of three weeks of observation as "necessary" for realistic simulation of the region. Or is this meant to refer to better constraints on the aerosol column more generally? In context, it appears to refer specifically to the South America-related results.

References:

Adebiyi, A. A., & Zuidema, P. (2016). The role of the southern African easterly jet in modifying the southeast Atlantic aerosol and cloud environments. *Quarterly Journal of the Royal Meteorological Society, 142*, 1574-1589.

Figure: